HEAVISIDE AND THE OPERATIONAL CALCULUS.*

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THE centenary of the birth of Oliver Heaviside last year has been the occasion of celebration by electrical engineers and physicists in this and other countries. In the discussions of his work much has been said about the Operational Calculus; and as the versions of its history which have been given both in these commemorative celebrations and in most of the textbooks of the subject are seriously incorrect, this may serve as an occasion to recount that history more correctly. The story in widest circulation is that the Operational Calculus was discovered by Heaviside (Boole being sometimes—and incorrectly—named as the discoverer of its applications to ordinary differential equations) and rejected by British mathematicians because of Heaviside's lack of rigour. The facts, as I shall show, are that the Calculus was well known in Britain and France before Heaviside's birth, and that the rejection of his paper had nothing to do with his use of symbolic methods. The first of these facts is sometimes recognised in German and American books—never, curiously enough, in those written in this country and France, where the calculus is invariably attributed to Heaviside; I hope to compensate for this lack of originality by what I hope is an accurate and, within limits of space, complete account of the early history of the subject, as well as by rescuing our predecessors from a charge of undue rigour made against them in no other context.

The essential idea of the Operational Calculus is that operations, almost invariably linear operations, on functions are treated as if they were algebraic quantities. The operators with which we shall be concerned almost exclusively are those derived from the differential operation D_t or D_x; where there is no fear of confusion the symbol of the independent variable will be omitted. Typical examples of the processes of the calculus are the solution of differential equations by factorisation; *e.g.* of

$$\frac{d^2u}{dt^2}+(a+b)\frac{du}{dt}+abu=f(t), \qquad (1)$$

by writing the operator $\{D^2+(a+b)D+ab\}$ as $(D+a)(D+b)$, which is legitimate if a and b are constants, and then either using two integrations, solving successively

$$(D+b)z=f(t) \quad \text{and} \quad (D+a)y=z, \qquad (2)$$

or using the method of partial fractions:

$$u=\frac{1}{(D+a)(D+b)}f(t)=\frac{1}{b-a}\left[\frac{1}{D+a}-\frac{1}{D+b}\right]f(t). \qquad (3)$$

Again, the equation

$$\frac{\partial^2u}{\partial x^2}-\frac{\partial^2u}{\partial y^2}=0 \quad \text{or} \quad (D_x{}^2-D_y{}^2)u=0 \qquad (4)$$

can be factorised to read $(D_x-D_y)(D_x+D_y)u=0$, and solved by successive integration. A more startling process, and the really critical one in the calculus, is the use of transcendental or irrational functions of D: thus we can solve

$$\frac{\partial^2u}{\partial x^2}=\frac{\partial u}{\partial t} \qquad (5)$$

* A lecture given to the London Mathematical Society, on January 18, 1951.

by writing it $\dfrac{\partial^2 u}{\partial x^2}=D_t u$ and the solution as

$$u=e^{-x\sqrt{D_t}}\phi(t)+e^{x\sqrt{D_t}}\psi(t). \qquad \text{.......................(6)}$$

Of course, this last form needs an interpretation of the symbols involved: various means of finding this have been used, of which the simplest is to expand in powers of D_t:

$$e^{x\sqrt{D_t}}\psi=\left\{\Sigma\frac{x^n D_t^{n/2}}{n!}\right\}\psi,$$

and to interpret D_t in some way consistent with the rule of indices, usually by the rule

$$\frac{D^m t^p}{\Gamma(p)}=\frac{t^{p-m}}{\Gamma(p-m)}. \qquad \text{.................................(7)}$$

To come to the history, we may quote one of the best-known books on the history of mathematics, by E. T. Bell, who presents the subject as follows. After some unimportant preliminaries, the operational calculus was discovered by Heaviside. His papers were rejected by the Royal Society because his methods were unorthodox, but since then it has been found that they are quite correct in the results they give because of the work of various " unsolicited rehabilitators " of Heaviside, Bromwich and others. In Bell's summing up:

" Following the trite pattern, the Heaviside tragi-comedy degenerated in three acts into broad farce: the Heaviside method was utter nonsense; it was right and could readily be justified; and everybody had known about it long before Heaviside used it, and it was in fact almost a trivial commonplace of classical analysis. . . . The supple and hoity-toity acrobats in the third act have been too busy balancing their dignity against their slip in the first act to notice what their gymnastics prove."

The hoity-toity acrobat referred to is Doetsch, who, in his book on the Laplace Transform (3) certainly is unduly disparaging in his remarks on Heaviside, but deserves no such condemnation as a historian. On the contrary, I shall show that there was no slip in the relevant act of Bell's play, but that Bell arrived too late at the theatre and failed to understand the plot.

The Operational Calculus may be regarded as primarily a discovery of the first quarter of the nineteenth century; but the earlier history of it is interesting. The earliest writer in whom we find the germs of the idea is, naturally enough, Leibnitz; he had noticed what he called " the analogy between differences and powers "; but this analogy seems to be not the one on which the modern symbolic calculus rests, that between the operation of differentiation and multiplication by a constant, but a less far-reaching and more awkward one between differentiation and raising a quantity to a power; to use suggestive notation, not that between $D^n u$ and $a^n u$, but that between $u^{(n)}$ and u^n, which is involved in Leibnitz' formula that $(uvw\ldots)^{(n)}$ expanded as a sum has the coefficients of $(u+v+w\ldots)^n$. This analogy Leibnitz extended to negative integers, and he proposed an extension to fractional indices, though his suggestion is very unclear to me. The next major writer is Lagrange (7). Lagrange attempted to free the differential calculus of all use of infinitesimals or limits by basing it on the possibility of expanding functions in power series, and on the calculus of finite differences. He was led to write Taylor's theorem in the form

$$f(x+h)-f(x)=(e^{hD}-1)f(x), \qquad \text{...........................(8)}$$

in our notation, and to derive formulae such as

$$\Delta^\lambda f(x) = (e^{hD} - 1)^\lambda f(x), \quad \text{.............................(8a)}$$

where $\Delta f(x) = f(x+h) - f(x)$ is the operator of the calculus of finite differences. Lagrange seems to have regarded these proofs as purely heuristic: and it seems likely that he was still thinking in terms of the Leibnitz analogy rather than of the modern one. For example, he wrote (8) not in the form in which it is given here but in the, strictly speaking incorrect, form

$$\Delta f(x) = \left(e^{h\frac{df}{dx}} - 1\right)$$

adding that after expanding the exponential as a power series we must change df^n into $d^n f$.

Laplace was the next to take up the question. He provided a rigorous proof of Lagrange's formulae (8) and (8a), by arguing that if the left-hand side is expanded as a series in x the coefficients are independent of the particular function f, and by taking $f = e^{ax}$ we get the correct coefficients and the formula. His major contribution was the " calcul des fonctions genératrices ". He assumed that a function could be represented in the form (**8**, pp. 83 *et seq.*)

$$f(x) = \int F(p)\, e^{px} dp, \quad \text{..................................(9)}$$

with limits of integration to be chosen suitably: and then

$$Df(x) = \int pF(p)\, e^{px} dp. \quad \text{..............................(10)}$$

He used this method to solve differential equations and difference equations (for which a series replaces the integral).

A useful distinction may be made here. A form of the operational calculus in which the operator is manipulated directly according to algebraic rules I shall call a *formal calculus*. One in which it is assumed that a function can be represented in a certain manner, and the operation of differentiation is made to correspond to ordinary algebraic operations on the terms of the series or integral in terms of which the function is represented I shall call a *representational calculus*. The distinction is analogous to that between synthetic and analytic geometry; or, better still, to that between a direct geometrical solution of a problem and one which is solved by transforming into another space and operating in the transform space. The distinction is not absolute: most formal calculuses use some representational methods as auxiliaries or to provide proofs. However, I shall not consider writers who make use of purely representational methods without discussing their connection with formal ones. Examples of these are those who discuss solutions of differential equations in power series, or Euler's investigations on solutions of differential equations in terms of integrals which were independent of Laplace and contain no symbolic discussion. The symbolic discussion in Laplace himself is slight, and his calculations were mainly carried out in terms of the representation. He gave a solution of the equation (5) by symbolic methods involving an expansion in a power series of D_x and evaluation of the terms as definite integrals. Similar methods were used by Poisson for the equation of heat conduction in three-dimensions and for the wave equation.

At the beginning of the nineteenth century formal calculations with D, $\int$, Δ and Σ were carried on by Arbogast and his followers; but they were based purely on analogy, and were not regarded as rigorous by their contemporaries. Indeed they were used to produce developments in series, some of which are patently absurd.

A major step in the theory of the formal calculus was taken by F. J. Servois (11), who was considered by contemporaries to have taken the " analogie entre les puissances et les différences " out of the sphere of mere analogy and made it mathematically satisfying ; in the process the meaning of the analogy was changed to the modern one. Servois pointed out that the operator D obeys the same laws as algebraic quantities, the laws

$$D(f+g)=Df+Dg, \quad D_xD_y=D_yD_x,$$

which he was the first to name the distributive and commutative laws respectively ; and he concluded that all laws and procedures valid for algebraic quantities were valid for them. He aimed to give a fresh means of carrying out Lagrange's programme of placing the differential calculus on a purely algebraic basis by basing it on the algebraic laws obeyed by the differential operator. It should be said of this programme, incidentally, that it was not as completely illusory as may appear : for it was admitted that considerations of limiting processes are essential for applications of the calculus to geometry and mechanics, and quite a large part of the analytical side of the differential calculus is purely algebraic. Leaving this aside, Servois must be considered to be the first man to have approached a formulation of the idea of a linear operator. One cannot help feeling surprised that this notion had escaped the great Lagrange, when one considers how much of his work on differential equations, his discovery of the adjoint equation, and his work in the theory of numbers are such that it is almost impossible for a modern mathematician to formulate them without using the concept of a linear operator : but an attentive reading of these parts of his work justifies the view that he did not have the general concept. In Servois, too, the concept of an operator is not clear : there is a confusion between operators and functions or algebraic quantities, which is illustrated by the fact that he wrote the distributive law $D(f+g)=Df+Dg$, considering this analogous to that for algebraic quantities, whereas the actual analogous form should be, say,

$$D_1(D_2+D_3)=D_1D_2+D_1D_3.$$

It was left to the British Mathematicians of the 1820's and 30's to clarify these ideas.

A systematic application of the differential calculus to differential equations was made by Brisson (12). His methods depend on rather complicated expansions in series ; but he seems to be the first author to discuss factorisation of symbolic expressions and its application to the solution of differential equations. His work is referred to very frequently in Cauchy's memoirs ; but it seems possible that Cauchy was writing of later work of his which may not have been published.

A major step forward in the development of the representational calculus was taken by Fourier (13). He gave and used systematically, in germ at any rate, two representational calculuses which have been the basis of all later ones. In fact, Fourier developed the theory of Fourier Series in order to give a representational calculus for periodic functions, and that of Fourier integrals to give one for non-periodic functions. He used both in the solution of equations of heat conduction, frequently writing the solutions in forms involving transcendental functions of D before using Fourier integrals or series to evaluate them ; and he proposed a calculus of fractional differentiation based on the Fourier integral. His calculus is greatly superior to that of Laplace in that it is possible to give an expression for the coefficients of the Fourier series, or the integrand in the Fourier integral, corresponding to a given function expressible by either of these means : whereas Laplace was not able to give

the form of the function $F(p)$ of (9) except where it was determined by the differential equation satisfied by $f(x)$. However, the symbolic methods occur in Fourier side by side with other methods from which it is rather hard to separate them, and which are used more frequently: his principal method was to find simple standard solutions and fit them together as sums and integrals to fulfil boundary conditions.

It is in the work of Cauchy that we find a first systematic representational operational calculus. In order to set up such a calculus Cauchy wrote the Fourier integral formula in its exponential form:

$$f(x)=\frac{1}{2\pi}\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} e^{iu(x-y)}f(y)\,dy,\,du \quad\ldots\ldots\ldots\ldots(12)$$

and for functions of the operator D gave the following formula:

$$\phi(D)f(x)=\frac{1}{2\pi}\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} \phi(iu)\,e^{iu(x-y)}f(y)\,dy\,du. \quad\ldots\ldots\ldots\ldots(13)$$

He also gave the corresponding formulae for functions of several variables. This is the essence of the modern form of the representational calculus. Cauchy's work on the subject is contained in a very large number of papers, covering most of his working life; they range from his prize-winning "Mémoire sur les Théories des Ondes" of 1815 (**14**a), in which there is little explicit use of symbolic methods, his "Sur l'analogie entre les puissances et les différences" of 1825 (**14**b), his "Mémoire sur le calcul intégral" presented in 1825 but published in 1850 (**14**c), to papers running up to 1850 representing various points of view on the subject. In the two last-mentioned papers the calculations are almost entirely symbolic: the representational method is mentioned as a general justification of the method, but is little called upon for the actual work. One example from the last memoir may be quoted: it is the solution of a problem very like those later considered by Heaviside, that of integrating

$$\frac{\partial u}{\partial t}-m^2\frac{\partial^2 u}{\partial x^2}+ru=0$$

for $a\leqslant x\leqslant b$ with boundary conditions

$$u(x,0)=f(x), \qquad \frac{\partial u}{\partial x}+Au=0 \text{ at } x=a, \qquad \frac{\partial u}{\partial x}+Bu=0 \text{ at } x=b.$$

The solution is written in the alternative forms

$$u=e^{(m^2D_x{}^2-r)t}f(x)=e^{x\Theta}\phi(t)+e^{-x\Theta}\lambda(t),$$

where

$$m\Theta=\surd(D_t+r),$$

and the difficulties due to the finite range of definition are overcome by ingenious manipulations. In later papers Cauchy used the purely symbolic methods to an increasing extent, and relied on the Calculus of Residues for evaluation of his formulae; the methods occur both in purely mathematical papers and in numerous papers on wave propagation and other questions of mathematical physics. The analysis is not always rigorous, but is always fairly critical. Cauchy's later point of view is indicated in his "Mémoire sur l'emploi des équations symboliques" (**14**d), in which he says, referring, as he always did, to Brisson as his predecessor in the use of these methods, that he had given a rigorous justification of the use of symbolic methods involving the functions of D and Δ as far as rational functions of these operators are concerned, but "one cannot say the same of formulae at which one arrives

in developing these functions in infinite series, as M. Brisson has proposed, or when with this author or M. Poisson one makes these operators enter under the sign of integration ". For testing the correctness of such procedures, he gives a simple prescription : substitute the results in the equations to be solved and verify the solution, taking care that series introduced remain convergent. In one sense nothing more satisfactory has been proposed to this day. Relying on such verification and the use of Residues, Cauchy developed the standard methods of modern operational calculus, including the method of factorisation of symbolic expressions and the method of expressing rational functions of symbolic operators involving both one and many variables as sums of partial fractions—the method which is today known as the Heaviside expansion theorem. He also used expansions in both convergent and divergent series, pointing out the dangers of the latter but justifying their use in some particular cases.

Fractional differentiation was used for the solution of differential equations by Liouville (15) as an application of his calculus of fractional differentiation. He solved a number of integral equations, and differential equations of the form

$$(mx^2+nx+p)\frac{d^2y}{dx^2}+(qx+r)\frac{dy}{dx}+sy=0.$$

Apart from these writers, and from a number of workers on the special problem of the factorisation of ordinary linear differential equations with non-constant coefficients, few or no French writers worked on symbolic methods between 1830 and about 1925 ; Cauchy's work seems to have been overlooked.

The other major centre for the development of symbolic methods was in this country. The earliest paper on these methods which I have traced is in the *Philosophical Transactions* for 1807, when the astronomer Brinkley extended some of Lagrange's formulae in the Calculus of Finite Differences ; he was followed by J. F. W. Herschel in 1816. In the interval, Babbage, Peacock and Herschel had won their struggle to replace the fluxional notation, which made Brinkley's work very cumbersome, and to introduce that of Leibnitz, " replacing the dot-age of Cambridge by the principles of pure d-ism ". Leibnitz' notation immediately suggests symbolic methods : and from the 30's to the 60's papers on symbolic methods poured into the mathematical periodicals : the *Cambridge*, the *Cambridge and Dublin*, and their successor the *Quarterly Journal of Mathematics* printed about six articles on the subject in most volumes of these years, and there was a steady flow in the *Philosophical Magazine*, the *Proceedings of the Royal Irish Academy*, and a respectable output of larger articles in the *Philosophical Transactions*.

The work on this subject was stimulated by the general attitude of the British school, which was algebraical and, on the whole, formalist. Peacock, though unclear in his ideas, was a pioneer in this line of thought. One of the earliest workers on symbolic methods, and one of the main inspirers of the British school, was D. F. Gregory, who died aged only 37. He formulated the principle that algebra is the science of formal symbols subject to certain laws of composition and axioms, to use modern phraseology, a view which was further expanded by de Morgan. Gregory took the view, following on Servois, that the fact that symbols obeyed the same laws as algebraic operations justified extending to them all identities valid for " algebraic quantities ", by which was meant the real numbers. This principle, called the principle of separations of symbols of operations from quantities, when applied to the operational calculus, was held by all members of the British school to be sufficient justification of their work. Gregory (**9a**) formulated

the algebraic laws more completely and clearly than Servois, and he added to the list the associative law, which was later so named by Hamilton. He criticised Fourier and Cauchy for timidity for relying on representational methods, a timidity which he explained by saying that only subsequent clarification had shown the validity of the principle of separation. The logic of the method was developed in the papers of Murphy (18), of Boole (19), and in Boole's book on *Differential Equations*. The importance of these papers, and particularly of Boole's, is that they were the first to define clearly the concept of an operator, of an inverse of an operator, and so on. Fourier methods were used frequently by these authors, but they were regarded only as a useful auxiliary for evaluating particular symbolic expressions and sometimes for proofs of general formulae. The work of these authors was not confined, as was that of Cauchy, to the solution of equations with constant coefficients, for which the Fourier methods are most useful, but extended to more general equations, and was proposed by Boole as a substitute, in his view an improvement, on the method of solution of ordinary differential equations in series. Such investigations made necessary the study of non-commutative operators.

It is most remarkable that all this work, with that of Cauchy, is quite overlooked by books on symbolic methods. As analysis it is open to considerable criticism. The great weakness is the neglect of questions of convergence; but if it is not better than some work done by modern symbolists, it is also no worse, and in dexterity of manipulation it outdoes a great deal of modern work, and anticipates much of it. It is quite likely that the results of many present-day researches can be found in these British papers or in those of Cauchy; the only general exceptions, I imagine, are work involving special functions not studied at that time, and work involving the convolution theorem for the Fourier or Laplace transform. A striking example of such anticipation is the study (20) by the Rev. Professor Graves of Dublin of operators obeying the law $\pi\rho - \rho\pi = 1$. His treatment is almost identical with that in modern texts on quantum theory, no less rigorous, and no more.

Some of the major British mathematicians of the time—de Morgan, Boole, Hamilton, Cayley—took part in this development. None seems to have questioned its bases, and it appears in most of the standard textbooks of the calculus (*e.g.* 9, 10). De Morgan wrote (10, p. 753) of the use of the method as applied to partial differential equations: "The use of the symbol of operation D_y as a constant with regard to another symbol of operation D_x is one of the severest trials to which the calculus of operations can be put, though following readily enough from the first principles of the science." His view was that in well-developed branches of mathematics thorough rigour is necessary, but that in newer branches an experimental and tentative approach is desirable.

The basic principle, that symbols satisfying the laws of algebra satisfy all identities obeyed by the real numbers, is true generally for identities involving only polynomials. It can be extended to rational functions provided inverse operations are carefully defined. It is not valid *per se* for functions of operators whose definition involves limiting processes; as Cauchy pointed out, it then needs to be supported by considerations of convergence. Oddly enough, Cauchy had in his representational interpretation a means of passing the limits mentioned, and within them had no need of a representational interpretation.

These difficulties, with others arising from unclarity about the domains and ranges of operators, showed themselves in occasional disputes about interpretations of operational formulae, in particular in an attack by Peacock on Liouville's theory of fractional differentiation and a subsequent controversy.

In the end the limitations of the method were bound to retard progress. By the 60's, the papers in the *Philosophical Transactions* show the floridness characteristic of an artistic decadence: long and unreadable treatises deal with operators subject to various commutation laws, and while these may have found their ostensible outlet in the solution of differential equations, it is more likely that their chief purpose was to satisfy the passion of the contemporary algebraists for long and complicated formulae, a passion more usually sated on invariant theory.

We now come to Heaviside's place in the story. Heaviside was born in May 1850; he was attracted to the study of telegraphy through Sir Charles Wheatstone, his uncle, and began a study of it, first experimental and then theoretical, as an employee of a telegraph company, which he later left to work on his own. He received no formal mathematical education apart from that at school in Camden Town, but he taught himself both mathematics and electromagnetic theory. In particular, he studied Boole's *Differential Equations* and Fourier's *Heat* ("the only entertaining book on mathematics"), and from these must have derived the ideas of his operational method. He taught himself to such good effect that he became a major contributor to Electromagnetic Theory. In awarding him an honorary degree in 1905 the University of Göttingen described him as "among the followers of Maxwell easily the first". As a mathematician he was gifted with great manipulative skill and with a genius for finding convenient methods of calculation. He simplified Maxwell's theory enormously; according to Hertz, the four equations known as Maxwell's were first given by Heaviside. He is one of the founders of vector analysis, and engaged in vigorous and lively polemics against quaternions. He wrote that an American schoolgirl was said to have defined quaternion as an ancient religious ceremony: "but this is a mistake; the ancients, unlike Professor Tait, knew not and did not worship quaternions" (*E.M.T.*, I, par. 100).

He described his attitude to mathematics as experimental, "bold and broad", and many entertaining passages of his writings are devoted to attacks on the purists, the Cambridge or conservatory mathematicians as he called them. He took part in the campaign for the reform of geometrical teaching. Of the teaching of Euclid, he said that it was a sad farce: in his school "two or three followed and were made temporarily into conceited logic-choppers, contradicting their parents; the effect upon most of the rest was disheartening and demoralising" (*Nature*, 1900).

As a result of his mathematical work on telegraphy, Heaviside laid the foundation of the modern theory of signals on telegraph wires, and in particular predicted that beneficial effects would follow from giving the wire a suitable quantity of self-inductance. This contradicted the prejudices of the Chief Electrician to the Post Office, and in consequence some of Heaviside's reports, written in conjunction with his brother, were suppressed by the Post Office; moreover, a series of articles of his in the *Electrician* was stopped on the grounds that no one read them; but he believed that there were other reasons. The result of this was that he later felt himself to be a rebel against official science, and had something of a persecution complex, which extended to contemporary mathematicians and mathematical physicists, although there is in fact little in their conduct towards him which could justify or even explain this feeling. He finally retired to Paignton, where he lived in eccentric isolation until 1925. Honours and offers of grants or posts came to him; many he refused as charity. In 1890 as a result of the recognition of his work by Kelvin, Rayleigh and Fitzgerald, he was elected a Fellow of the Royal Society.

These facts help to explain his attitude to mathematics. In addition, it

must be remembered that he was primarily a physicist—though he had an intense interest in some parts of pure mathematics—and was not very widely read in mathematics. He seems not to have been particularly fluent in foreign languages. (" Foreigners ", he wrote, " have a gift for languages and have invented a large number of lingos . . . let them give us poor islanders the benefit of their skill by doing all their best work into English." *E.M.T.*, III, p. 53.)

The Operational Calculus was first used by him for the solutions of the equations of telegraph theory : second order partial differential equations of hyperbolic type with constant coefficients and with initial values of the function given at $t=0$. Two papers of his, " On Operational Methods in Physical Mathematics," Parts I and II, were published in the *Proceedings of the Royal Society* in 1892 and 1893. Part III was rejected in 1894. It is this rejection which has led to the legend that the Operational Calculus, discovered by Heaviside, was rejected by his contemporaries.

The published papers and Chapter VIII of Part II of *E.M.T.*, which reproduces the three papers in brief, illustrate what Heaviside meant by experimental mathematics. He made no claim to rigour, and manipulated infinite series, convergent or divergent, in all respects as if they were finite sums. He knew that his methods could lead to error : this he considered part of their charm. It is quite incorrect to regard him as an innovator in this : he was only following the attitude of his teachers, the textbooks from which he had taught himself, with less caution than they had shown ; he was, in fact, as a mathematician, typical of the British mathematics of the 1850's.

The papers on " Operational Methods in Physical Mathematics " do not contain a systematic account of Operational Methods ; moreover, they do not contain applications to physics, or even, for the most part, to the solution of differential equations. They are contributions to the theory of the gamma, exponential and Bessel functions. Now criticism of the rigour of a solution of a differential equation is always slightly pointless : the solution can be checked, and any way of finding it is legitimate. Criticism of mathematical rigour in works on physics is also held to be out of place, with less reason ; the physicist's intuition is supposed, more accurately in the nineteenth century than now, to be able to guide him. It is questionable whether the acceptance of two standards of rigour is always beneficial either to pure or to applied mathematics : but as long as he does not find himself on the R 101 or the Tacoma suspension bridge at a critical moment, victim of an engineer's false assumption of a uniqueness theorem, the mathematician may well let other sciences follow the practices which experience has commended to them. Heaviside's papers, however, being purely mathematical and contributions to the Theory of Functions, a subject in which results are useless without tolerably rigorous proof, need to be judged by stricter standards.

Now Heaviside's favourite method for the interpretation of an operational symbol was to expand the symbol in an ascending or descending power series of the operator D_t, often with fractional powers, and to substitute for $D_t^{-\alpha}$ the function $t^\alpha/\Gamma(\alpha+1)$, and for D_t^n the zero function if n is a positive integer. In his symbolism all functions were generated by operation with functions of D_t from the unit function, written 1 by him, which is 1 for positive and zero for negative t. The result of this procedure was, often enough, a divergent series : in many cases one that today would be considered a valid asymptotic expansion.

A critical reader of the first two papers will be more surprised that they were published than that the third was rejected. If he bears in mind the earlier history of the subject we will certainly conclude that it was this use of divergent series that led to rejection ; and this guess I have been able to

verify through the kindness of the Royal Society, who have allowed me to see the referee's report. The explanation of the more favourable treatment of the first two papers is simple: they were not refereed. According to the Records, at that time and for years after no paper of a Fellow submitted to the *Proceedings* was refereed (though all *Transactions* papers were refereed), and special action must have been taken on Heaviside's third paper. This is confirmed by a story which was told to Sir Edmund Whittaker by one of those concerned (21):

"There was a sort of tradition that a Fellow of the Royal Society could print almost anything he liked in the *Proceedings* untroubled by referees: but when Heaviside had published two papers on his symbolic methods, we felt the line had to be drawn somewhere, so we put a stop to it."

Heaviside himself asserted that a reason for the rejection was a prejudice against him because of his attacks on quaternions. This is most unlikely: none of those concerned had any strong interest in quaternions, and they were attacked by others, including Cayley and Kelvin.

The referee's report makes it clear that his objections apply as much to the first two parts, which deal mainly with the exponential and binomial series and Bessel's functions of order zero, as to the third which deals with Bessel's functions of any order; and, because they are simpler, takes most of his examples from Parts I and II.

For the history of the symbolic calculus, the most significant feature of the report is the complete absence of any mention of it. It can safely be assumed that the referee either took it for granted or, if he had any private doubts about it, considered it so well established that he would no more have felt it his duty to object to its use than a referee of intuitionist leanings would today feel it right to object to the use of the principle of the excluded middle in a paper on classical analysis. His objections turn entirely on the use made of divergent series. It does not appear that he objected to the use of divergent series in all circumstances: he considered that some of those obtained by Heaviside might be of use for numerical calculation. His objection is to the fact that the equivalence of a divergent and a convergent series or of two divergent series is nowhere defined, and that they are manipulated with no justification for their use. He complains that no definition is given of the sum of a divergent series, but infers that the definition used is that it is equal to the sum up to the smallest term in the divergent part with an error of less than the first term omitted. It may be mentioned, incidentally, that this definition is to be found in Laplace's *Probabilités* and in papers of Cauchy and Stokes.

He quotes the series given for e^{-x}. A series for e^{-x} was given in Part II, 52; in Part III this is "shortly dismissed as wrong, apparently in consequence of another 'formula' for e^{-x} which is obtained in Part III.... He says in effect, let us consider *

$$u = -\frac{x^{r-1}}{\lfloor r-1} + \frac{x^r}{\lfloor r} - \frac{x^{r+1}}{\lfloor r+1} + \dots$$

continued to infinity both ways. It *obviously* satisfies $\frac{du}{dx} + u = 0$, therefore $u = Ae^{-x}$. When $r=0$ the series is e^{-x} and when $r=1$ it is $-e^{-x}$, therefore let us try $u = \cos r\pi e^{-x}$. Taking certain special values of r and x he shows that there is approximate numerical equality between that part of the series which begins with the least term in the divergent part and $\cos r\pi e^{-x}$ and concludes

* This series had been given by Riemann and Cayley. It is discussed in G. H. Hardy's *Divergent Series*.

that the whole series is equivalent to cos $r\pi e^{-x}$. Later on he alters this result again. Comment on the above process appears to me to be needless."

Further, the referee says that it seems that the author was "ignorant of the modern developments of the theory of linear differential equations which have followed from Herr Fuchs' paper (*Crelle*, vol. 66): and in consequence, if this view is correct, he is trying to find a 'royal road' to results which have already been established by exact reasoning." His conclusion is that the results of the paper may or may not be correct: the methods by which they have been found make them valueless.

It may be asked whether the referee would have been more lenient if he had known the modern theory of asymptotic series. The report shows explicitly only knowledge of Stokes' theory of asymptotic series, which is contained in the definition of its sum which, the referee infers, is the one used by Heaviside. The modern theory was established by Poincaré in 1886 in a paper on linear differential equations in *Acta Mathematica*: I think it unlikely that the referee knew of this, but he may conceivably be referring to this in his remarks about the "modern developments" of the Fuchs theory, which is otherwise a little hard to understand. However, it would in my view have made little difference whether the Poincaré theory was known to the referee or not: for most of Heaviside's methods and some of his results will not fit any theory of series. Chapter VIII of Vol. II of *E.M.T.* shows that he had no consistent theory of them. His procedure was to find asymptotic expansions by any methods, and to test them by numerical calculations for small values of the argument. For example, in § 431 he gives convergent and asymptotic expansions for the function equivalent of the symbol $(1+D^{-1})^n$. (The functions involved are in the notation of Whittaker and Watson, *Modern Analysis*, Chapter XVI, the confluent hypergeometric functions $x^{-\frac{1}{2}}e^{-\frac{1}{2}x}M_{\frac{1}{2}-n,0}(x)$.) Heaviside tested his expansions for various n, and considered them correct for $n=\pm\frac{1}{2}$, $\pm\frac{1}{4}$, but incorrect, because his numerical checks failed, for other n, in particular $n=-1$. Now for all I know the series are correct in all cases: but let us take $n=-1$. We have

$$\begin{aligned}(1+D^{-1})^{-1} &= 1-D^{-1}+D^{-2}- \\ &= 1-t+\frac{t^2}{2!}-\ldots=e^{-t},\end{aligned}$$

and

$$\frac{D}{1+D}=D-D^2+D^3-\ldots=0,$$

and we arrive at the result $e^{-t}=0$, which Heaviside rejects. Now this is an example of a correct asymptotic expansion: any function such that $t^n f(t)\to 0$ for all n as $t\to\infty$ has a zero asymptotic expansion for positive t. Clearly numerical tests for small t are useless for checking an asymptotic expansion. Earlier (§ 429) we find the formula (No. 28)

$$\sum_{r=-\infty}^{\infty}\frac{(1-1)^{r+\alpha}}{\Gamma(r+\alpha+1)}x^{r+\alpha}=1,$$

in the usual notation, which the most hardened summer of divergent series would find awkward. It shook even Heaviside: but he says: "The apparent numerical unintelligibility is no necessary bar to the use of (28) as a working and transforming formula. It often turns up." Later in the chapter, when series fail to satisfy his numerical tests he modifies his definition of the sum, taking fractions of the term after the smallest into the calculation.

This matter would not be worth so much attention but for the repeated statements by eminent mathematicians that Heaviside was one of the greatest

creative mathematicians of the nineteenth century and that his methods never led him to error. These statements arise mainly, I think, from admiration of his great achievements in physics and their contrast with his unhappy life. I share this admiration : but I feel that an accurate statement of his actual achievements is high enough praise for this famous man, and that it is a false generosity which makes him a posthumous gift of other men's work and reputations.

In fact, the concentration on Heaviside has had unfortunate effects on the teaching of operational calculus : it has come to be regarded with more suspicion than is justified, and even electrical engineers, like van der Pol, who use the legend as a stick to beat the mathematicians, take good care not to use Heaviside's methods, but to substitute for them the use of the Laplace Transform. In fact, for quite a large class of differential equations the use of symbolic methods can be justified without such extraneous aids.

It is pleasant to record that in spite of the numerous gibes at them in his work Heaviside remained on good terms with many " conservatory mathematicians ". One may add that if his remarks about mathematicians (" Even Cambridge mathematicians deserve justice " ; " Cambridge is where good mathematicians go when they die ") are often quoted, while those on anti-mathematical electrical engineers are not, it is because the former are good-humoured, the latter very bitter. In 1913 W. H. Young exchanged letters with him, discussing asymptotic series and fractional differentiation ; they did not get far, having little in common save a belief that British mathematics was in a bad way. From 1913 to 1920 he corresponded with Bromwich, first about divergent series and then about his symbolic methods : only Bromwich's letters to him seem to be available, at the library of the Institution of Electrical Engineers. Bromwich described to Heaviside the methods he was using to take the place of the symbolic methods, based on contour integration : and Heaviside, then aged 70, learned to use them.

Mathematicians have made varying judgments of the contribution made by Heaviside to the Operational Calculus. On balance, my own conclusion is that there was little new in his methods and that his main contribution was to apply them to a new field and to make their use a necessity in electrical engineering. His use of them in the solution of the differential equations of electromagnetic theory is not open to the criticisms which apply to his work on asymptotic series, for in the former field results are easily checked. From the mathematical point of view there are perhaps two new features in his work ; (*a*) the idea that all functions could be generated from the unit function by symbolic operation with functions of D ; and (*b*) the use of the operational calculus for functions defined for positive t, and taken to be zero for negative t.

There are, in fact, three principal types of operational calculus for the symbol D ; * and I suggest that the following terminology for them would be convenient and do less violence to history than many such attributions :

(i) The *Fourier Operational Calculus*, for periodic functions. This is the calculus used by electrical engineers in the study of A.C. circuits. As a representational calculus it depends on the Fourier series ; functions are expressed in the form

$$f(t) = \sum_{-\infty}^{\infty} a_n e^{int},$$

* One could also distinguish other calculuses for different operators, and even for D. Thus the calculus used by nineteenth-century British mathematicians for solution of ordinary differential equations is a calculus for $x\,D_x$, whose representational form is the theory of solution in power series (cf. **24**).
The substitution $x = e^t$ reduces this to one similar to (ii).

and the results of operation on them by

$$\phi(D)f(t) = \Sigma\phi(in)a_n e^{int}.$$

(ii) The *Cauchy Operational Calculus* for functions defined over $(-\infty, \infty)$. For functions small enough at infinity, the representational form is that given by Cauchy in formulae (12) and (13). For functions large at infinity this will no longer work; some generalisation of the Fourier transform formula must be used. The generalised Fourier transforms of Titchmarsh (22) will serve for functions of exponential order at infinity.

(iii) The *Heaviside Operational Calculus* for functions over $(0, \infty)$. For this Calculus we can use either the Titchmarsh generalised Fourier transforms, or, what is more usual and comes to the same thing, the Laplace transform. The transform of a function $f(t)$ is

$$F(p) = \int_0^\infty f(t)e^{-pt}dt,$$

provided $f(t)$ is of exponential order at infinity, and this has the inversion formula

$$f(t) = \frac{1}{2\pi i}\int_{\alpha-i\infty}^{\alpha+i\infty} F(p)e^{pt}dp.$$

This was substantially Bromwich's method of dealing with Heaviside's Calculus, and it is closely allied to Cauchy's. However, there is a striking difference between the first two and the third calculus, which can be expressed operationally in the following manner. Suppose we have the functional identity

$$\phi(u) = \theta(u)\eta(u),$$

then we would expect to have the operational identity

$$\phi(D)f = \theta(D)\eta(D)f, \quad \text{.................................}(15)$$

whenever the symbols have a meaning. This holds in (i) and (ii), but not in general in (iii). A striking instance is that

$$\frac{1}{D}Df(t) = \int_0^t Df\,dt = f(t) - f(0), \qquad D\frac{1}{D}f(t) = D\int_0^t f(u)\,du = f(t),$$

or, again, that since

$$\begin{aligned} e^{-hD}f(t) &= f(t-h) && \text{if } t \geqslant h, \\ &= 0 && \text{if } t < h, \\ e^{hD}f(t) &= f(t+h) && \text{if } t \geqslant 0 \end{aligned}$$

so that $\qquad e^{-hD}\,.\,e^{+hD}f(t) = f(t), \qquad$ if $t \geqslant 0$.

If formula (15) does not hold, a perfect operational representation is impossible. In fact, it is easily seen by integration by parts that the Laplace Transformation of $Df(t)$ is $pF(p) - f(0)$, not $pF(p)$ as would be necessary for a perfect representational method. The failure of (15) is the source of a number of errors in the symbolic calculus. A representational calculus can only be set up for a restricted class of operational symbols.

Heaviside's symbolic form for a function $f(t)$ is $DF(D)$. This was pointed out by himself in *E.M.T.*, Vol III, par. 526; he arrived at it by a most interesting intuitive argument. The formula is ascribed by electrical engineers to Carson—a clear indication that they do not read Heaviside.

The later history of the Operational Calculus can be dealt with here only very briefly. The two main trends have been the setting up of mathematically sound formal operational calculuses for functional operators and the investiga-

tion of the Fourier and Laplace Transform methods by a number of writers. The first of these trends is one of the major advances in modern mathematics. Its essence lies in the removal of the main defect of the formal calculuses of the earlier nineteenth century: the purely algebraic ideas of these writers were supplemented by essential convergence notions by considering the functions on which the operations are performed as points in a generalised space, a function space, in which a topology which allows the definition of limiting processes is introduced. This method was applied first to integral operators, because these operators are bounded, that is, apply to every element of the space, unlike the differential operators which apply to certain of, for example, the continuous functions. Later it was extended to unbounded operators, and today a large part of the symbolic calculus for D can be incorporated in this theory.

The Fourier Transform or Laplace Transform methods are substantially equivalent to the modern form of these operational methods, but can be used with greater flexibility. The usual method of applying them today to the solution of a differential equation is to deduce from the equation the equation which is satisfied by the transform of the unknown function, to solve this, and to use the inversion formulae to find the unknown function from its transform. This method is logically better, and also more reliable in practice, than the Cauchy-Fourier method of differentiation under the sign of integration or summation. It can be said, in criticism of it, that the added rigour is principally concerned with a matter of secondary importance. If the method is carried out with the most rigorous logic it will show only that if there is a solution belonging to the class of functions to which the transformation applies, then it must have a certain form. It therefore has the effect of establishing the uniqueness of a solution within a definite class of functions, but it does not provide an existence proof, it does not show that the form actually verifies the equation. The only author who has attempted a general investigation of this point is R. V. Churchill (**23**), and the complication of his results suggests that for satisfying oneself that one has a solution the old prescription of Cauchy, " substitute it and see ", remains the best. The difficulty that the class of functions to which the transformations apply is restricted in size at infinity can be overcome by taking the transformation over a finite region, and then examining what happens to the terms introduced by the boundary of the region when it tends to infinity. This method gives proofs of uniqueness of solution more general than those given by any other method.

For many problems, notably those differential equations in which the final solution depends on the initial values only over a finite region, it is possible to justify completely the use of operational methods, and it is not necessary to resort to Fourier transform methods for each particular solution, nor even, in some cases, for general proofs. For other equations uniqueness depends on restrictions on size at infinity, and it is probably impossible to set up a general purely symbolic method for them.

The modern literature of the subject is enormous, is of very varied quality, and includes countless rediscoveries of known results. Much of it is concerned with applications to electrical circuits or other engineering problems.

The outstanding mystery is the apparent forgetting of the earlier work; even Bromwich seems not to have known it. It is to be hoped that the future will see a better understanding of the history of the symbolic calculus, and a return to the use of symbolic methods as an autonomous discipline in the many cases where this is possible.

In conclusion, I wish to express my gratitude to the Institution of Electrical Engineers for allowing me to consult Heaviside's papers and to the Royal Society for permission to see the referee's report.

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J. L. B. C.

1685. The odds against each of the four players at Bridge holding a complete suit are said to be 2,235,753,911,732,487,297,923,559,999 to one, and my calculations approximate to this stupendous figure.

Reports of similar freak deals crop up quite regularly, and your correspondent instances three within the past seven years. But can they be credited? If such frequent reports are true, the irresistible conclusion is that the odds quoted above are erroneous, and that the accepted mathematical theories of probability must be scrapped when applied to card deals. We must assume unknown factors which influence the fall of the cards and lessen the purely chance probability by many millions per cent.—*Sunday Times*, December 10, 1950. [Per Mr. G. E. Crawford.]